

SCIENCE

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ADDRESS OF THE PRESIDENT OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE¹

THE outstanding feature of this meeting must be the fact that we are here—in Australia. It is the function of a president to tell the Association of advances in science, to speak of the universal rather than of the particular or the temporary. There will be other opportunities of expressing the thoughts which this event must excite in the dullest heart, but it is right that my first words should take account of those achievements of organization and those acts of national generosity by which it has come to pass that we are assembled in this country. Let us, too, on this occasion, remember that all the effort, and all the goodwill, that binds Australia to Britain would have been powerless to bring about such a result had it not been for those advances in science which have given man a control of the forces of nature. For we are here by virtue of the feats of genius of individual men of science, giant-variations from the common level of our species; and since I am going soon to speak of the significance of individual variation, I can not introduce that subject better than by calling to remembrance the line of pioneers in chemistry, in physics, and in engineering, by the working of whose rare—or, if you will, abnormal—intellects a meeting of the British Association on this side of the globe has been made physically possible.

I have next to refer to the loss within

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the year of Sir David Gill, a former president of this association, himself one of the outstanding great. His greatness lay in the power of making big foundations. He built up the Cape Observatory; he organized international geodesy; he conceived and carried through the plans for the photography of the whole sky, a work in which Australia is bearing a conspicuous part. Astronomical observation is now organized on an international scale, and of this great scheme Gill was the heart and soul. His labors have ensured a base from which others will proceed to discovery otherwise impossible. His name will be long remembered with veneration and gratitude.

As the subject of the addresses which I am to deliver here and in Sydney I take *Heredity*. I shall attempt to give the essence of the discoveries made by Mendelian or analytical methods of study, and I shall ask you to contemplate the deductions which these physiological facts suggest in application both to evolutionary theory at large and to the special case of the natural history of human society.

Recognition of the significance of heredity is modern. The term itself in its scientific sense is no older than Herbert Spencer. Animals and plants are formed as pieces of living material split from the body of the parent organisms. Their powers and faculties are fixed in their physiological origin. They are the consequence of a genetic process, and yet it is only lately that this genetic process has become the subject of systematic research and experiment. The curiosity of naturalists has of course always been attracted to such problems; but that accurate knowledge of genetics is of paramount importance in any attempt to understand the nature of living things has only been realized quite lately even by naturalists, and with casual

exceptions the laity still know nothing of the matter. Historians debate the past of the human species, and statesmen order its present or profess to guide its future as if the animal man, the unit of their calculations, with his vast diversity of powers, were a homogeneous material, which can be multiplied like shot.

The reason for this neglect lies in ignorance and misunderstanding of the nature of variation; for not until the fact of congenital diversity is grasped, with all that it imports, does knowledge of the system of hereditary transmission stand out as a primary necessity in the construction of any theory of evolution, or any scheme of human polity.

The first full perception of the significance of variation we owe to Darwin. The present generation of evolutionists realizes perhaps more fully than did the scientific world in the last century that the theory of evolution had occupied the thoughts of many and found acceptance with not a few before ever the "Origin" appeared. We have come also to the conviction that the principle of natural selection can not have been the chief factor in delimiting the species of animals and plants, such as we now with fuller knowledge see them actually to be. We are even more sceptical as to the validity of that appeal to changes in the conditions of life as direct causes of modification, upon which latterly at all events Darwin laid much emphasis. But that he was the first to provide a body of fact demonstrating the variability of living things, whatever be its causation, can never be questioned.

There are some older collections of evidence, chiefly the work of the French school, especially of Godron²—and I would mention also the almost forgotten essay of

² "De l'Espèce et des Races dans les Etres Organisés," 1859.

Wollaston³—these however are only fragments in comparison. Darwin regarded variability as a property inherent in living things, and eventually we must consider whether this conception is well founded; but postponing that inquiry for the present, we may declare that with him began a general recognition of variation as a phenomenon widely occurring in nature.

If a population consists of members which are not alike but differentiated, how will their characteristics be distributed among their offspring? This is the problem which the modern student of heredity sets out to investigate. Formerly it was hoped that by the simple inspection of embryological processes the modes of heredity might be ascertained, the actual mechanism by which the offspring is formed from the body of the parent. In that endeavor a noble pile of evidence has been accumulated. All that can be made visible by existing methods has been seen, but we come little if at all nearer to the central mystery. We see nothing that we can analyze further—nothing that can be translated into terms less inscrutable than the physiological events themselves. Not only does embryology give no direct aid, but the failure of cytology is, so far as I can judge, equally complete. The chromosomes of nearly related creatures may be utterly different both in number, size and form. Only one piece of evidence encourages the old hope that a connection might be traceable between the visible characteristics of the body and those of the chromosomes. I refer of course to the accessory chromosome, which in many animals distinguishes the spermatozoon about to form a female in fertilization. Even it however can not be claimed as the cause of sexual differentiation, for it may be paired in forms closely allied to those in which it is

unpaired or accessory. The distinction may be present or wanting, like any other secondary sexual character. Indeed, so long as no one can show consistent distinctions between the cytological characters of somatic tissues in the same individual we can scarcely expect to perceive such distinctions between the chromosomes of the various types.

For these methods of attack we now substitute another, less ambitious, perhaps, because less comprehensive, but not less direct. If we can not see how a fowl by its egg and its sperm gives rise to a chicken or how a sweet pea from its ovule and its pollen grain produces another sweet pea, we at least can watch the system by which the differences between the various kinds of fowls or between the various kinds of sweet peas are distributed among the offspring. By thus breaking the main problem up into its parts we give ourselves fresh chances. This analytical study we call Mendelian because Mendel was the first to apply it. To be sure, he did not approach the problem by any such line of reasoning as I have sketched. His object was to determine the genetic definiteness of species; but though in his writings he makes no mention of inheritance it is clear that he had the extension in view. By cross-breeding he combined the characters of varieties in mongrel individuals and set himself to see how these characters would be distributed among the individuals of subsequent generations. Until he began this analysis nothing but the vaguest answers to such a question had been attempted. The existence of any orderly system of descent was never even suspected. In their manifold complexity human characteristics seemed to follow no obvious system, and the fact was taken as a fair sample of the working of heredity.

Misconception was especially brought in by describing descent in terms of "blood."

³ "On the Variation of Species," 1856.

The common speech uses expressions such as consanguinity, pure-blooded, half-blood, and the like, which call up a misleading picture to the mind. Blood is in some respects a fluid, and thus it is supposed that this fluid can be both quantitatively and qualitatively diluted with other bloods, just as treacle can be diluted with water. Blood in primitive physiology being the peculiar vehicle of life, at once its essence and its corporeal abode, these ideas of dilution and compounding of characters in the commingling of bloods inevitably suggest that the ingredients of the mixture once combined are inseparable, that they can be brought together in any relative amounts, and in short that in heredity we are concerned mainly with a quantitative problem. Truer notions of genetic physiology are given by the Hebrew expression "seed." If we speak of a man as "of the blood-royal" we think at once of plebeian dilution, and we wonder how much of the royal fluid is likely to be "in his veins"; but if we say he is "of the seed of Abraham" we feel something of the permanence and indestructibility of that germ which can be divided and scattered among all nations, but remains recognizable in type and characteristics after 4,000 years.

I know a breeder who had a chest containing bottles of colored liquids by which he used to illustrate the relationships of his dogs, pouring from one to another and titrating them quantitatively to illustrate their pedigrees. Galton was beset by the same kind of mistake when he promulgated his "Law of Ancestral Heredity." With modern research all this has been cleared away. The allotment of characteristics among offspring is not accomplished by the exudation of drops of a tincture representing the sum of the characteristics of the parent organism, but by a process of *cell-division*, in which numbers of these char-

acters, or rather the elements upon which they depend, are sorted out among the resulting germ-cells in an orderly fashion. What these elements, or *factors* as we call them, are we do not know. That they are in some way directly transmitted by the material of the ovum and of the spermatozoon is obvious, but it seems to me unlikely that they are in any simple or literal sense material particles. I suspect rather that their properties depend on some phenomenon of arrangement. However that may be, analytical breeding proves that it is according to the distribution of these genetic factors, to use a non-committal term, that the characters of the offspring are decided. The first business of experimental genetics is to determine their number and interactions, and then to make an analysis of the various types of life.

Now the ordinary genealogical trees, such as those which the stud-books provide in the case of the domestic animals, or the Heralds' College provides in the case of man, tell nothing of all this. Such methods of depicting descent can not even show the one thing they are devised to show—purity of "blood." For at last we know the physiological meaning of that expression. An organism is pure-bred when it has been formed by the union in fertilization of two germ-cells which are alike in the factors they bear; and since the factors for the several characteristics are independent of each other, this question of purity must be separately considered for each of them. A man, for example, may be pure-bred in respect of his musical ability and cross-bred in respect of the color of his eyes or the shape of his mouth. Though we know nothing of the essential nature of these factors, we know a good deal of their powers. They may confer height, color, shape, instincts, powers both of mind and body; indeed, so many of the attributes

which animals and plants possess that we feel justified in the expectation that with continued analysis they will be proved to be responsible for most if not all of the differences by which the varying individuals of any species are distinguished from each other. I will not assert that the greater differences which characterize distinct species are due generally to such independent factors, but that is the conclusion to which the available evidence points. All this is now so well understood, and has been so often demonstrated and expounded, that details of evidence are now superfluous.

But for the benefit of those who are unfamiliar with such work let me briefly epitomize its main features and consequences. Since genetic factors are definite things, either present in or absent from any germ-cell, the individual may be either "pure-bred" for any particular factor or its absence, if he is constituted by the union of two germ-cells both possessing or both destitute of that factor. If the individual is thus pure, all his germ-cells will in that respect be identical, for they are simply bits of the similar germ-cells which united in fertilization to produce the parent organism. We thus reach the essential principle, that an organism can not pass on to offspring a factor which it did not itself receive in fertilization. Parents, therefore, which are both destitute of a given factor can only produce offspring equally destitute of it; and, on the contrary, parents both pure-bred for the presence of a factor produce offspring equally pure-bred for its presence. Whereas the germ-cells of the pure-bred are all alike, those of the cross-bred, which results from the union of dissimilar germ-cells, are mixed in character. Each positive factor segregates from its negative opposite, so that some germ-cells carry the factor and some do not. Once the factors have been identified by their

effects, the average composition of the several kinds of families formed from the various matings can be predicted.

Only those who have themselves witnessed the fixed operations of these simple rules can feel their full significance. We come to look behind the simulacrum of the individual body and we endeavor to disintegrate its features into the genetic elements by whose union the body was formed. Set out in cold general phrases such discoveries may seem remote from ordinary life. Become familiar with them and you will find your outlook on the world has changed. Watch the effects of segregation among the living things with which you have to do—plants, fowls, dogs, horses, that mixed concourse of humanity we call the English race, your friends' children, your own children, yourself—and however firmly imagination be restrained to the bounds of the known and the proved, you will feel something of that range of insight into nature which Mendelism has begun to give. The question is often asked whether there are not also in operation systems of descent quite other than those contemplated by the Mendelian rules. I myself have expected such discoveries, but hitherto none have been plainly demonstrated. It is true we are often puzzled by the failure of a parental type to reappear in its completeness after a cross—the merino sheep or the fantail pigeon, for example. These exceptions may still be plausibly ascribed to the interference of a multitude of factors, a suggestion not easy to disprove; though it seems to me equally likely that segregation has been in reality imperfect. Of the descent of quantitative characters we still know practically nothing. These and hosts of difficult cases remain almost untouched. In particular the discovery of E. Baur, and the evidence of Winkler in regard to his "graft hybrids," both showing that the

sub-epidermal layer of a plant—the layer from which the germ-cells are derived—may bear exclusively the characters of a part only of the soma, give hints of curious complications, and suggest that in plants at least the interrelations between soma and gamete may be far less simple than we have supposed. Nevertheless, speaking generally, we see nothing to indicate that qualitative characters descend, whether in plants or animals, according to systems which are incapable of factorial representation.

The body of evidence accumulated by this method of analysis is now very large, and is still growing fast by the labors of many workers. Progress is also beginning along many novel and curious lines. The details are too technical for inclusion here. Suffice it to say that not only have we proof that segregation affects a vast range of characteristics, but in the course of our analysis phenomena of most unexpected kinds have been encountered. Some of these things twenty years ago must have seemed inconceivable. For example, the two sets of sex organs, male and female, of the same plant may not be carrying the same characteristics; in some animals characteristics, quite independent of sex, may be distributed solely or predominantly to one sex; in certain species the male may be breeding true to its own type, while the female is permanently mongrel, throwing off eggs of a distinct variety in addition to those of its own type; characteristics, essentially independent, may be associated in special combinations which are largely retained in the next generation, so that among the grandchildren there is numerical preponderance of those combinations which existed in the grandparents—a discovery which introduces us to a new phenomenon of polarity in the organism.

We are accustomed to the fact that the

fertilized egg has a polarity, a front and hind end for example; but we have now to recognize that it, or the primitive germinal cells formed from it, may have another polarity shown in the groupings of the parental elements. I am entirely sceptical as to the occurrence of segregation solely in the maturation of the germ-cells,⁴ preferring at present to regard it as a special case of that patch-work condition we see in so many plants. These mosaics may break up, emitting bud-sports at various cell-divisions, and I suspect that the great regularity seen in the F_2 ratios of the cereals, for example, is a consequence of very late segregation, whereas the excessive irregularity found in other cases may be taken to indicate that segregation can happen at earlier stages of differentiation.

The paradoxical descent of color-blindness and other sex-limited conditions—formerly regarded as an inscrutable caprice of nature—has been represented with approximate correctness, and we already know something as to the way, or perhaps I should say ways, in which the determination of sex is accomplished in some of the forms of life—though, I hasten to add, we have no inkling as to any method by which that determination may be influenced or directed. It is obvious that such discoveries have bearings on most of the problems, whether theoretical or practical, in which animals and plants are concerned. Permanence or change of type, perfection of type, purity or mixture of race, “racial development,” the succession of forms, from being vague phrases expressing matters of degree, are now seen to be capable of acquiring physiological meanings, already to some extent assigned with precision. For

⁴ The fact that in certain plants the male and female organs respectively carry distinct factors may be quoted as almost decisively negating the suggestion that segregation is confined to the reduction division.

the naturalist—and it is to him that I am especially addressing myself to-day—these things are chiefly significant as relating to the history of organic beings—the theory of evolution, to use our modern name. They have, as I shall endeavor to show in my second address to be given in Sydney, an immediate reference to the conduct of human society.

I suppose that every one is familiar in outline with the theory of the origin of species which Darwin promulgated. Through the last fifty years this theme of the natural selection of favored races has been developed and expounded in writings innumerable. Favored races certainly can replace others. The argument is sound, but we are doubtful of its value. For us that debate stands adjourned. We go to Darwin for his incomparable collection of facts. We would fain emulate his scholarship, his width and his power of exposition, but to us he speaks no more with philosophical authority. We read his scheme of evolution as we would those of Lucretius or of Lamarck, delighting in their simplicity and their courage. The practical and experimental study of variation and heredity has not merely opened a new field; it has given a new point of view and new standards of criticism. Naturalists may still be found expounding teleological systems⁵ which would have delighted Dr.

⁵ I take the following from the abstract of a recent Croonian Lecture "On the Origin of Mammals" delivered to the Royal Society: "In Upper Triassic times the larger Cynodonts preyed upon the large Anomodont, *Kannemeyeria*, and carried on their existence so long as these Anomodonts survived, but died out with them about the end of the Trias or in Rhætic times. The small Cynodonts, having neither small Anomodonts nor small Cotylosaurs to feed on, were forced to hunt the very active long-limbed Thecodonts. The greatly increased activity brought about that series of changes which formed the mammals—the flexible skin with hair, the four-chambered heart and

Pangloss himself, but at the present time few are misled. The student of genetics knows that the time for the development of theory is not yet. He would rather stick to the seed-pan and the incubator.

In face of what we now know of the distribution of variability in nature the scope claimed for natural selection in determining the fixity of species must be greatly reduced. The doctrine of the survival of the fittest is undeniable so long as it is applied to the organism as a whole, but to attempt by this principle to find value in all definiteness of parts and functions, and in the name of science to see fitness everywhere is mere eighteenth-century optimism. Yet it was in application to the parts, to the details of specific difference, to the spots on the peacock's tail, to the coloring of an orchid flower, and hosts of such examples, that the potency of natural selection was urged with the strongest emphasis. Shorn of these pretensions the doctrine of the survival of favored races is a truism, helping scarcely at all to account for the diversity of species. Tolerance plays almost as considerable a part. By these admissions almost the last shred of that teleological fustian with which Victorian philosophy loved to clothe the theory of evolution is destroyed. Those who would proclaim that whatever is right will be wise henceforth to base this faith frankly on the impregnable rock of superstition and to abstain from direct appeals to natural fact.

My predecessor said last year that in physics the age is one of rapid progress and profound scepticism. In at least as high

warm blood, the loose jaw with teeth for mastication, an increased development of tactile sensation and a great increase of cerebrum. Not improbably the attacks of the newly-evolved Cynodont or mammalian type brought about a corresponding evolution in the Pseudosuchian Thecodonts which ultimately resulted in the formation of Dinosaurs and Birds." Broom, R., *Proc. Roy. Soc. B.*, 87, p. 88.

a degree this is true of biology, and as a chief characteristic of modern evolutionary thought we must confess also to a deep but irksome humility in presence of great vital problems. Every theory of evolution must be such as to accord with the facts of physics and chemistry, a primary necessity to which our predecessors paid small heed. For them the unknown was a rich mine of possibilities on which they could freely draw. For us it is rather an impenetrable mountain out of which the truth can be chipped in rare and isolated fragments. Of the physics and chemistry of life we know next to nothing. Somehow the characters of living things are bound up in properties of colloids, and are largely determined by the chemical powers of enzymes, but the study of these classes of matter has only just begun. Living things are found by a simple experiment to have powers undreamed of, and who knows what may be behind?

Naturally we turn aside from generalities. It is no time to discuss the origin of the Mollusca or of Dicotyledons, while we are not even sure how it came to pass that *Primula obconica* has in twenty-five years produced its abundant new forms almost under our eyes. Knowledge of heredity has so reacted on our conceptions of variation that very competent men are even denying that variation in the old sense is a genuine occurrence at all. Variation is postulated as the basis of all evolutionary change. Do we then as a matter of fact find in the world about us variations occurring of such a kind as to warrant faith in a contemporary progressive evolution? Till lately most of us would have said "yes" without misgiving. We should have pointed, as Darwin did, to the immense range of diversity seen in many wild species, so commonly that the difficulty is to define the types themselves. Still more conclusive seemed the profusion of forms in

the various domesticated animals and plants, most of them incapable of existing even for a generation in the wild state, and therefore fixed unquestionably by human selection. These, at least, for certain, are new forms, often distinct enough to pass for species, which have arisen by variation. But when analysis is applied to this mass of variation the matter wears a different aspect. Closely examined, what is the "variability" of wild species? What is the natural fact which is denoted by the statement that a given species exhibits much variation? Generally one of two things: either that the individuals collected in one locality differ among themselves; or perhaps more often that samples from separate localities differ from each other. As direct evidence of variation it is clearly to the first of these phenomena that we must have recourse—the heterogeneity of a population breeding together in one area. This heterogeneity may be in any degree, ranging from slight differences that systematists would disregard, to a complex variability such as we find in some moths, where there is an abundance of varieties so distinct that many would be classified as specific forms but for the fact that all are freely breeding together. Naturalists formerly supposed that any of these varieties might be bred from any of the others. Just as the reader of novels is prepared to find that any kind of parents might have any kind of children in the course of the story, so was the evolutionist ready to believe that any pair of moths might produce any of the varieties included in the species. Genetic analysis has disposed of all these mistakes. We have no longer the smallest doubt that in all these examples the varieties stand in a regular descending order, and that they are simply terms in a series of combinations of factors separately transmitted, of which each may be present or absent.

The appearance of contemporary variability proves to be an illusion. Variation from step to step in the series must occur either by the addition or by the loss of a factor. Now, of the origin of new forms *by loss* there seems to me to be fairly clear evidence, but of the *contemporary acquisition* of any new factor I see no satisfactory proof, though I admit there are rare examples which may be so interpreted. We are left with a picture of variation utterly different from that which we saw at first. Variation now stands out as a definite physiological event. We have done with the notion that Darwin came latterly to favor, that large differences can arise by accumulation of small differences. Such small differences are often mere ephemeral effects of conditions of life, and as such are not transmissible; but even small differences, when truly genetic, are factorial like the larger ones, and there is not the slightest reason for supposing that they are capable of summation. As to the origin or source of these positive separable factors, we are without any indication or surmise. By their effects we know them to be definite, as definite, say, as the organisms which produce diseases; but how they arise and how they come to take part in the composition of the living creature so that when present they are treated in cell-division as constituents of the germs, we can not conjecture.

It was a commonplace of evolutionary theory that at least the domestic animals have been developed from a few wild types. Their origin was supposed to present no difficulty. The various races of fowl, for instance, all came from *Gallus bankiva*, the Indian jungle-fowl. So we are taught; but try to reconstruct the steps in their evolution and you realize your hopeless ignorance. To be sure there are breeds, such as Black-red Game and Brown Leghorns,

which have the colors of the jungle-fowl, though they differ in shape and other respects. As we know so little as yet of the genetics of shape, let us assume that those transitions could be got over. Suppose, further, as is probable, that the absence of the maternal instinct in the Leghorn is due to loss of one factor which the jungle-fowl possesses. So far we are on fairly safe ground. But how about White Leghorns? Their origin may seem easy to imagine, since white varieties have often arisen in well-authenticated cases. But the white of White Leghorns is not, as white in nature often is, due to the loss of the color-elements, but to the action of something which inhibits their expression. Whence did that something come? The same question may be asked respecting the heavy breeds, such as Malays or Indian Game. Each of these is a separate introduction from the East. To suppose that these, with their peculiar combs and close feathering, could have been developed from preexisting European breeds is very difficult. On the other hand, there is no wild species now living any more like them. We may, of course, postulate that there was once such a species, now lost. That is quite conceivable, though the suggestion is purely speculative. I might thus go through the list of domesticated animals and plants of ancient origin and again and again we should be driven to this suggestion, that many of their distinctive characters must have been derived from some wild original now lost. Indeed, to this unsatisfying conclusion almost every careful writer on such subjects is now reduced. If we turn to modern evidence the case looks even worse. The new breeds of domestic animals made in recent times are the carefully selected products of recombination of preexisting breeds. Most of the new varieties of cultivated plants are the outcome of deliberate crossing. There is gen-

erally no doubt in the matter. We have pretty full histories of these crosses in gladiolus, orchids, cineraria, begonia, calceolaria, pelargonium, etc. A very few certainly arise from a single origin. The sweet pea is the clearest case, and there are others which I should name with hesitation. The cyclamen is one of them, but we know that efforts to cross cyclamens were made early in the cultural history of the plant, and they may very well have been successful. Several plants for which single origins are alleged, such as the Chinese primrose, the dahlia and tobacco, came to us in an already domesticated state, and their origins remain altogether mysterious. Formerly single origins were generally presumed, but at the present time numbers of the chief products of domestication, dogs, horses, cattle, sheep, poultry, wheat, oats, rice, plums, cherries, have in turn been accepted as "polyphyletic" or, in other words, derived from several distinct forms. The reason that has led to these judgments is that the distinctions between the chief varieties can be traced as far back as the evidence reaches, and that these distinctions are so great, so far transcending anything that we actually know variation capable of effecting, that it seems pleasanter to postpone the difficulty, relegating the critical differentiation to some misty antiquity into which we shall not be asked to penetrate. For it need scarcely be said that this is mere procrastination. If the origin of a form under domestication is hard to imagine, it becomes no easier to conceive of such enormous deviations from type coming to pass in the wild state. Examine any two thoroughly distinct species which meet each other in their distribution, as, for instances, *Lychnis diurna* and *vespertina* do. In areas of overlap are many intermediate forms. These used to be taken to be transitional steps, and the specific distinctness

of *vespertina* and *diurna* was on that account questioned. Once it is known that these supposed intergrades are merely mongrels between the two species the transition from one to the other is practically beyond our powers of imagination to conceive. If both these can survive, why has their common parent perished? Why when they cross do they not reconstruct it instead of producing partially sterile hybrids? I take this example to show how entirely the facts were formerly misinterpreted.

When once the idea of a true-breeding—or, as we say, homozygous—type is grasped, the problem of variation becomes an insistent oppression. What can make such a type vary? We know, of course, one way by which novelty can be introduced—by crossing. Cross two well-marked varieties—for instance, of Chinese primula—each breeding true, and in the second generation by mere recombination of the various factors which the two parental types severally introduced, there will be a profusion of forms, utterly unlike each other, distinct also from the original parents. Many of these can be bred true, and if found wild would certainly be described as good species. Confronted by the difficulty I have put before you, and contemplating such amazing polymorphism in the second generation from a cross in *Antirrhinum*, Lotsy has lately with great courage suggested to us that all variation may be due to such crossing. I do not disguise my sympathy with this effort. After the blind complacency of conventional evolutionists it is refreshing to meet so frank an acknowledgment of the hardness of the problem. Lotsy's utterance will at least do something to expose the artificiality of systematic zoology and botany. Whatever might or might not be revealed by experimental breeding, it is certain that without such tests we are merely guessing when we pro-

fess to distinguish specific limits and to declare that this is a species and that a variety. The only definable unit in classification is the homozygous form which breeds true. When we presume to say that such and such differences are trivial and such others valid, we are commonly embarking on a course for which there is no physiological warrant. Who could have foreseen that the apple and the pear—so like each other that their botanical differences are evasive—could not be crossed together, though species of *antirrhinum* so totally unlike each other as *majus* and *molle* can be hybridized, as Baur has shown, without a sign of impaired fertility? Jordan was perfectly right. The true-breeding forms which he distinguished in such multitudes are real entities, though the great systematists, dispensing with such laborious analysis, have pooled them into arbitrary Linnean species, for the convenience of collectors and for the simplification of catalogues. Such pragmatistical considerations may mean much in the museum, but with them the student of the physiology of variation has nothing to do. These "little species," finely cut, true-breeding, and innumerable mongrels between them, are what he finds when he examines any so-called variable type. On analysis the semblance of variability disappears, and the illusion is shown to be due to segregation and recombination of series of factors on predetermined lines. As soon as the "little species" are separated out they are found to be fixed. In face of such a result we may well ask with Lotsy, is there such a thing as spontaneous variation anywhere? His answer is that there is not.

Abandoning the attempt to show that positive factors can be added to the original stock, we have further to confess that we can not often actually prove variation by loss of factor to be a real phenomenon.

Lotsy doubts whether even this phenomenon occurs. The sole source of variation, in his view, is crossing. But here I think he is on unsafe ground. When a well-established variety like "Crimson King" primula, bred by Messrs. Sutton in thousands of individuals, gives off, as it did a few years since, a salmon-colored variety, "Coral King," we might claim this as a genuine example of variation by loss. The new variety is a simple recessive. It differs from "Crimson King" only in one respect, the loss of a single color-factor, and, of course, bred true from its origin. To account for the appearance of such a new form by any process of crossing is exceedingly difficult. From the nature of the case there can have been no cross since "Crimson King" was established, and hence the salmon must have been concealed as a recessive from the first origin of that variety, even when it was represented by very few individuals, probably only by a single one. Surely, if any of these had been heterozygous for salmon this recessive could hardly have failed to appear during the process of self-fertilization by which the stock would be multiplied, even though that selfing may not have been strictly carried out. Examples like this seem to me practically conclusive.⁶ They can be challenged, but not, I think, successfully. Then again in regard to those variations in number and division of parts which we call meristic, the reference of these to original cross-breeding is surely barred by the circumstances in which they often occur. There remain also the rare examples mentioned already in which a single wild origin may with much confidence be assumed. In spite of repeated trials, no one has yet succeeded in crossing the sweet pea with any other

⁶The numerous and most interesting "mutations" recorded by Professor T. H. Morgan and his colleagues in the fly, *Drosophila*, may also be cited as unexceptionable cases.

leguminous species. We know that early in its cultivated history it produced at least two marked varieties which I can only conceive of as spontaneously arising, though, no doubt, the profusion of forms we now have was made by the crossing of those original varieties. I mention the sweet pea thus prominently for another reason, that it introduces us to another though subsidiary form of variation, which may be described as a *fractionation* of factors. Some of my Mendelian colleagues have spoken of genetic factors as permanent and indestructible. Relative permanence in a sense they have, for they commonly come out unchanged after segregation. But I am satisfied that they may occasionally undergo a quantitative disintegration, with the consequence that varieties are produced intermediate between the integral varieties from which they were derived. These disintegrated conditions I have spoken of as subtraction—or reduction—stages. For example, the Picotee sweet pea, with its purple edges, can surely be nothing but a condition produced by the factor which ordinarily makes the fully purple flower, quantitatively diminished. The pied animal, such as the Dutch rabbit, must similarly be regarded as the result of partial defect of the chromogen from which the pigment is formed, or conceivably of the factor which effects its oxidation. On such lines I think we may with great confidence interpret all those intergrading forms which breed true and are not produced by factorial interference.

It is to be inferred that these fractional degradations are the consequence of irregularities in segregation. We constantly see irregularities in the ordinary meristic processes, and in the distribution of somatic differentiation. We are familiar with half segments, with imperfect twinning, with leaves partially petaloid, with petals

partially sepaloid. All these are evidences of departures from the normal regularity in the rhythms of repetition, or in those waves of differentiation by which the qualities are sorted out among the parts of the body. Similarly, when in segregation the qualities are sorted out among the germ-cells in certain critical cell-divisions, we can not expect these differentiating divisions to be exempt from the imperfections and irregularities which are found in all the grosser divisions that we can observe. If I am right, we shall find evidence of these irregularities in the association of unconformable numbers with the appearance of the novelties which I have called fractional. In passing let us note how the history of the sweet pea belies those ideas of a continuous evolution with which we had formerly to contend. The big varieties came first. The little ones have arisen later, as I suggest by fractionation. Presented with a collection of modern sweet peas how prettily would the devotees of continuity have arranged them in a graduated series, showing how every intergrade could be found, passing from the full color of the wild Sicilian species in one direction to white, in the other to the deep purple of "Black Prince," though happily we know these two to be among the earliest to have appeared.

Having in view these and other considerations which might be developed, I feel no reasonable doubt that though we may have to forego a claim to variations by addition of factors, yet variation both by loss of factors and by fractionation of factors is a genuine phenomenon of contemporary nature. If then we have to dispense, as seems likely, with any addition from without we must begin seriously to consider whether the course of evolution can at all reasonably be represented as an unpacking of an original complex which contained

within itself the whole range of diversity which living things present. I do not suggest that we should come to a judgment as to what is or is not probable in these respects. As I have said already, this is no time for devising theories of evolution, and I propound none. But as we have got to recognize that there has been an evolution, that somehow or other the forms of life have arisen from fewer forms, we may as well see whether we are limited to the old view that evolutionary progress is from the simple to the complex, and whether after all it is conceivable that the process was the other way about. When the facts of genetic discovery become familiarly known to biologists, and cease to be the preoccupation of a few, as they still are, many and long discussions must inevitably arise on the question, and I offer these remarks to prepare the ground. I ask you simply to open your minds to this possibility. It involves a certain effort. We have to reverse our habitual modes of thought. At first it may seem rank absurdity to suppose that the primordial form or forms of protoplasm could have contained complexity enough to produce the divers types of life. But is it easier to imagine that these powers could have been conveyed by extrinsic additions? Of what nature could these additions be? Additions of material can not surely be in question. We are told that salts of iron in the soil may turn a pink hydrangea blue. The iron can not be passed on to the next generation. How can the iron multiply itself? The power to assimilate the iron is all that can be transmitted. A disease-producing organism like the pebrine of silkworms can in a very few cases be passed on through the germ-cells. Such an organism can multiply and can produce its characteristic effects in the next generation. But it does not become part of the invaded host, and we can not conceive it taking part

in the geometrically ordered processes of segregation. These illustrations may seem too gross; but what refinement will meet the requirements of the problem, that the thing introduced must be, as the living organism itself is, capable of multiplication and of subordinating itself in a definite system of segregation? That which is conferred in variation must rather itself be a change, not of material, but of arrangement, or of motion. The invocation of additions extrinsic to the organism does not seriously help us to imagine how the power to change can be conferred, and if it proves that hope in that direction must be abandoned, I think we lose very little. By the re-arrangement of a very moderate number of things we soon reach a number of possibilities practically infinite.

That primordial life may have been of small dimensions need not disturb us. Quantity is of no account in these considerations. Shakespeare once existed as a speck of protoplasm not so big as a small pin's head. To this nothing was added that would not equally well have served to build up a baboon or a rat. Let us consider how far we can get by the process of removal of what we call "epistatic" factors, in other words those that control, mask, or suppress underlying powers and faculties. I have spoken of the vast range of colors exhibited by modern sweet peas. There is no question that these have been derived from the one wild bi-color form by a process of successive removals. When the vast range of form, size and flavor to be found among the cultivated apples is considered it seems difficult to suppose that all this variety is hidden in the wild crab-apple. I can not positively assert that this is so, but I think all familiar with Mendelian analysis would agree with me that it is probable, and that the wild crab contains presumably inhibiting elements which the

cultivated kinds have lost. The legend that the seedlings of cultivated apples become crabs is often repeated. After many inquiries among the raisers of apple seedlings I have never found an authentic case—once only even an alleged case, and this on inquiry proved to be unfounded. I have confidence that the artistic gifts of mankind will prove to be due not to something added to the make-up of an ordinary man, but to the absence of factors which in the normal person inhibit the development of these gifts. They are almost beyond doubt to be looked upon as *releases* of powers normally suppressed. The instrument is there, but it is "stopped down." The scents of flowers or fruits, the finely repeated divisions that give its quality to the wool of the merino, or in an analogous case the multiplicity of quills to the tail of the fantail pigeon, are in all probability other examples of such releases. You may ask what guides us in the discrimination of the positive factors and how we can satisfy ourselves that the appearance of a quality is due to loss. It must be conceded that in these determinations we have as yet recourse only to the effects of dominance. When the tall pea is crossed with the dwarf, since the offspring is tall we say that the tall parent passed a factor into the cross-bred which makes it tall. The pure tall parent had two doses of this factor; the dwarf had none; and since the cross-bred is tall we say that one dose of the dominant tallness is enough to give the full height. The reasoning seems unanswerable. But the commoner result of crossing is the production of a form intermediate between the two pure parental types. In such examples we see clearly enough that the full parental characteristics can only appear when they are homozygous—formed from similar germ-cells, and that one dose is insufficient to produce either

effect fully. When this is so we can never be sure which side is positive and which negative. Since, then, when dominance is incomplete we find ourselves in this difficulty, we perceive that the amount of the effect is our only criterion in distinguishing the positive from the negative, and when we return even to the example of the tall and dwarf peas the matter is not so certain as it seemed. Professor Cockerell lately found among thousands of yellow sunflowers one which was partly red. By breeding he raised from this a form wholly red. Evidently the yellow and the wholly red are the pure forms, and the partially red is the heterozygote. We may then say that the yellow is YY with two doses of a positive factor which inhibits the development of pigment; the red is yy , with no dose of the inhibitor; and the partially red are Yy , with only one dose of it. But we might be tempted to think the red was a positive characteristic, and invert the expressions, representing the red as RR , the partly red as Rr , and the yellow as rr . According as we adopt the one or the other system of expression we shall interpret the evolutionary change as one of loss or as one of addition. May we not interpret the other apparent new dominants in the same way? The white dominant in the fowl or in the Chinese primula can inhibit color. But may it not be that the original colored fowl or primula had two doses of a factor which inhibited this inhibitor? The pepper moth, *Amphidasys betularia*, produced in England about 1840 a black variety, then a novelty, now common in certain areas, which behaves as a full dominant. The pure blacks are no blacker than the cross-bred. Though at first sight it seems that the black *must* have been something added, we can without absurdity suggest that the normal is the term in which two doses of

inhibitor are present, and that in the absence of one of them the black appears.

In spite of seeming perversity, therefore, we have to admit that there is no evolutionary change which in the present state of our knowledge we can positively declare to be not due to loss. When this has been conceded it is natural to ask whether the removal of inhibiting factors may not be invoked in alleviation of the necessity which has driven students of the domestic breeds to refer their diversities to multiple origins. Something, no doubt, is to be hoped for in that direction, but not until much better and more extensive knowledge of what variation by loss may effect in the living body can we have any real assurance that this difficulty has been obviated. We should be greatly helped by some indication as to whether the origin of life has been single or multiple. Modern opinion is, perhaps, inclining to the multiple theory, but we have no real evidence. Indeed, the problem still stands outside the range of scientific investigation, and when we hear the spontaneous formation of formaldehyde mentioned as a possible first step in the origin of life, we think of Harry Lauder in the character of a Glasgow schoolboy pulling out his treasures from his pocket—"That's a wassher—for makkin' motor cars!"

As the evidence stands at present all that can be safely added in amplification of the evolutionary creed may be summed up in the statement that variation occurs as a definite event often producing a sensibly discontinuous result; that the succession of varieties comes to pass by the elevation and establishment of sporadic groups of individuals owing their origin to such isolated events; and that the change which we see as a nascent variation is often, perhaps always, one of loss. Modern research lends not the smallest encouragement or sanction to the view that gradual evolution occurs by the

transformation of masses of individuals, though that fancy has fixed itself on popular imagination. The isolated events to which variation is due are evidently changes in the germinal tissues, probably in the manner in which they divide. It is likely that the occurrence of these variations is wholly irregular, and as to their causation we are absolutely without surmise or even plausible speculation. Distinct types once arisen, no doubt a profusion of the forms called species have been derived from them by simple crossing and subsequent recombination. New species may be now in course of creation by this means, but the limits of the process are obviously narrow. On the other hand, we see no changes in progress around us in the contemporary world which we can imagine likely to culminate in the evolution of forms distinct in the larger sense. By intercrossing dogs, jackals and wolves, new forms of these types can be made, some of which may be species, but I see no reason to think that from such material a fox could be bred in indefinite time, or that dogs could be bred from foxes.

Whether science will hereafter discover that certain groups can by peculiarities in their genetic physiology be declared to have a prerogative quality justifying their recognition as species in the old sense, and that the differences of others are of such a subordinate degree that they may in contrast be termed varieties, further genetic research alone can show. I myself anticipate that such a discovery will be made, but I can not defend the opinion with positive conviction.

Somewhat reluctantly, and rather from a sense of duty, I have devoted most of this address to the evolutionary aspects of genetic research. We can not keep these things out of our heads, though sometimes we wish we could. The outcome, as you will have seen, is negative, destroying much that

till lately passed for gospel. Destruction may be useful, but it is a low kind of work. We are just about where Boyle was in the seventeenth century. We can dispose of alchemy, but we can not make more than a quasi-chemistry. We are awaiting our Priestley and our Mendeléeff. In truth it is not these wider aspects of genetics that are at present our chief concern. They will come in their time. The great advances of science are made like those of evolution, not by imperceptible mass-improvement, but by the sporadic birth of penetrative genius. The journeymen follow after him, widening and clearing up, as we are doing along the track that Mendel found.

WILLIAM BATESON

MORPHOLOGY OF THE BACTERIA (VIBRIO AND SPIRILLUM), AN EARLY RE-SEARCH,¹—THE INTESTINAL FLORA

BIOLOGY presents few more fascinating pictures than that which portrays the early development of microscopic research in relation to what is now recognized as the science of bacteriology, and in our anxiety to pursue the utilitarian side of the subject it behooves us not to forget the work of the early pioneer naturalists who gave us the first glimpse of the foundation stones of what has come to be one of the most important departments of biological science. Did time permit, I should like to dwell in detail upon the early work of Leeuwenhoek,² Müller,³ Bory-de Saint Vincent, and later Ehrenberg⁴ and Dujardin,⁵

¹ The research with which this paper deals came to light during a review of the work performed by various authors upon the intestinal flora of men and the lower orders of animals, and it is hoped that the subject will prove of sufficient interest to justify the writer in bringing it to the attention of the Society of American Bacteriologists.

² *Transactions Royal Society*, 1675-1683.

³ "Animalia Infusoria," 1773.

⁴ "Die Infusionsthierchen als Valkom Organism," 1838; *Verhandl. der Berl. Acad.*, 1839.

⁵ "Historie Naturelle des Zoophytes," 1841.

respectively, 1839-1841—the latter of whom were the first to attempt a systematic classification of the bacteria—made doubly difficult—for until this time and for some years later these microorganisms or animalcula, as they were then termed, were included among the Infusoria and were so classified.

Authorities have credited Perty, 1852, and Robin, 1853, as the first observers to suggest a vegetal nature of these organisms. In a recent review of the scientific correspondence between Joseph Leidy and Spencer F. Baird, late secretary of the Smithsonian Institution, in 1847-1849, a letter from Leidy to Baird in 1847 attracted my attention. In it he observes that he is in the midst of an investigation upon the structure of the alimentary canal and the chemical processes of digestion, and desires a series of insects from the mountainous regions of Pennsylvania, where Baird then lived, upon which to pursue his investigation, the results of which he would communicate later through a report to the Philadelphia Academy of Natural Science.

Curious to observe the character of this research, upon reference to the Academy's Proceedings, we find in *October, 1849*, Leidy presented a paper with the following preamble:

From the opinion so frequently expressed that contagious diseases and some others might have their origin and reproductive character through the agency of cryptogamic spores, which, from their minuteness and lightness, are so easily conveyed from place to place through the atmosphere, by means of the gentlest Zephyr, or even the evaporation continually taking place from the earth's surface; and from the numerous facts already presented of the presence of cryptogamic vegetation in many cutaneous diseases and upon other diseased surfaces, I was led to reflect upon the possibility of plants of this description existing in healthy animals, as a natural condition; or at least apparently so, as in the case of entozoa. Upon considering that the conditions essential to vegetable growth were the same as those indispensable to animal life, I felt convinced that entophyta would be found in healthy living animals, as well, and probably as frequently, as entozoa. The constant presence of mycodermatoid filaments growing upon the human teeth, the teeth of the ox, sheep, pig, etc., favored this idea, and accordingly

I instituted a course of investigation, which led to the discovery of several well-characterized forms of vegetable growth, of which, at present, I will give but a short description, for the purpose of establishing priority, and propose giving a more detailed account of them, with figures, in the second volume of the journal.

Then follows a description of various new genera and species of cryptogamic vegetation, growing upon the basement membrane of the small intestine of the myriapod *Julus marginatus* (Say), and upon the exterior of the entozoa—*Acaris infecta*, infesting this insect—another new genera of entophyta allied to the mycodermata. He further observes:

Centipede, Millipede, Thousandleg

The three genera of entophyta of which I have now spoken are all so constantly found in *Julus marginatus* that I look upon it as a natural condition, and should I hereafter meet with an individual without them, I will consider it a rare exception, because in one hundred and sixteen individuals which I have examined during the past thirteen months, in all seasons, and at all ages and sizes of from one up to three inches of the animal, I have invariably found them. It can not be supposed that these are developed and grow after death, because I found them always immediately upon killing the animal. Whilst the legs of fragments of the animals were yet moving upon my table, or one half of the body even walking, I have frequently been examining the plants growing upon part of the intestinal canal of the same individual. And upon the entozoa these entophyta will be frequently found growing, whilst the former are actively moving about. I found among others an ascaris three lines long, which had no less than twenty-three individuals of *Enterobrus* (parasitic), averaging a line in length, besides a quantity of the other two genera, growing upon it, and yet it moved about in so lively a manner that it did not appear the least incommoded by its load of vegetation. This specimen I have preserved in a glass cell in Goadby's solution, and exhibit it to the academy.

The genus *Julus* is an extensive one, and its species are found in all the great parts of the globe, and as their habits are the same, the conditions for the production of the entophyta will be the same, and I think I do not go too far when I say they will be constantly found throughout the genus in any part of the world, so that naturalists and

other, may, upon examination, readily verify or contradict the statements which I have this evening presented.

Then follow to us these interesting observations:

From these facts we may perceive that we may have entophyta in luxurious growth within living animals without affecting their health, which is further supported by my having detected mycodermatoid filaments in the cœcum of six young and healthy rats, examined immediately after death, although they existed in no other part of the body. These filaments were minute, simple and inarticulate, measuring from $1/5,000$ to $1/1,428$ inch in length by $1/16,000$ of an inch in breadth. With them were also found two species of *Vibrio*.

Even those moving filamentary bodies belonging to the genus *Vibrio*, are of the character of algous vegetation. Their movement is no objection to this opinion, for much higher confervæ, as the *Oscillatorias*, are endowed with inherent power of movement, not very unlike that of the *Vibrio*, and indeed the movement of the latter appears to belong to one stage of its existence. Thus, in the toad (*Bufo americanus*), in the stomach and small intestine, there exist simple, delicate, filamentary bodies, which are of three different kinds. One is exceedingly minute, forms a single spiral, is endowed with a power of rapid movement, and appears to be the *Spirillum undula* of Ehrenberg; the second is an exceedingly minute, straight and short filament, with a movement actively molecular in character, and is probably the *Vibrio lineola* of the same author; the third consists of straight, motionless filaments, measuring $1/1,125$ inch long by $1/15,000$ broad; some were, however, twice or even thrice this length; but then I could always detect one or two articulations, and these, in all their characters, excepting want of movement, resemble the *Vibrio*. In the rectum of the same animal, the same filamentary bodies are found, with myriads of *Bodo intestinalis*; but the third species, or longest of the filamentary bodies, have increased immensely in numbers, and now possess the movement peculiar to the *Vibrio lineola*, which, however, does not appear to be voluntary, but reactionary; they bend and pursue a straight course, until they meet with some obstacle, when they instantly move in the opposite direction, either extremity forward.

These observations were published in 1849, and it is of interest to note that ten years

elapsed before M. Davaine in 1859⁶ made the same observation in almost identical language suggesting the vegetal nature of the *Vibrio*—its alliance to the Algæ and especially the Confervæ.

Leidy continues in the same number of the Proceedings:

But it must not be understood that these facts militate against the hypothesis of the production of contagious diseases through the agency of cryptogamia. It is well established that there are microscopic cryptogamia capable of producing and transmitting disease, as in the case of the Muscardine, etc., as that there are innocuous and poisonous fungi. In many instances it is difficult to distinguish their character, whether as cause or effect, as upon diseased surfaces, in *Tinea capitis*, apthous ulcers, etc. In a post-mortem examination, in which I assisted Dr. Horner,⁷ a few weeks since, 28 hours after death, in moderately cool weather, we found the stomach in a much softened condition. In the mucus of the stomach, I detected myriads of mycodermatoid filaments, resembling those growing upon the teeth; simple, floating, inarticulate and measuring from 1/7,000 to 1/520 of an inch in length by 1/25,000 of an inch in breadth. It is possible that they may have been the cause of the softened condition; but I would prefer thinking that swallowed mycodermatoid filaments from the teeth, finding an excellent nidus in the softening stomach, rapidly grew and reproduced themselves. In the healthy human stomach these do not exist.

In the stomach of a diabetic patient, I found so very few that they probably did not grow there, but were swallowed in the saliva.

A note is appended to this report:

Note:—Since the above went to press, Dr. Leidy announced to the academy that he had discovered two new species of the entophyte *Enterobrus*; one of them, *E. spiralis*, growing in the small intestine *Julus pusillus*; the other, *E. attenuatus*, growing more or less profusely with a second species of *Cladophytum*, *C. clavatum*, in the ventriculus of the coleopterous insect, *Passalus cornutus*. Thus has been established the law "that plants may grow in the interior of the healthy animal as a normal condition," and a new field has

⁶ *Rend. Comp.*, Paris, 1859, V., 58, 59; also "Traite des Entozoaires," Paris, 1860.

⁷ W. E. Horner, professor of anatomy, University of Pennsylvania, 1849.

been presented for the investigation of the *Cryptogame-naturalist*. (See forthcoming number of the Proceedings.)⁸

Also in December, 1849, appears:

Besides the foregoing I have found numerous free or floating entophyta in the contents, usually of the *posterior part of the alimentary canal*, in *mammalia*, *aves*, *reptilia*, *pisces*, *mollusca*, *insecta*, etc. These, at present, I do not feel at liberty to describe as new or peculiar, from my want of acquaintance with cryptogamic botany. A number of them, I have no doubt, if not peculiar, at least continue to grow luxuriantly in the intestinal canal; such are various *Mycoderma*, etc.; others very probably are swallowed with the food, and pass from the intestinal canal unchanged. Numerous drawings of these I exhibit to the Academy, and purpose leaving them to future investigation, or to the consideration of cryptogamic botanists, being a field well worthy of their researches. I also have a number of others, the character of which is peculiarly entophytic; but these I have not yet studied out nor figured, but hope to present descriptions of them to the academy in a very short time.

These researches upon the morphology and vegetal nature of the *Vibrio* and *Spirillum*; the suggestion of polymorphism, much dilated upon by later observers; the enunciation of a new law of the general existence of a parasitic intestinal flora of cryptogamic vegetation existing throughout the animal kingdom as a normal condition; the pathological significance of the presence of germs upon diseased surfaces, as to cause or effect; the suggestion of the inherent resistance of healthy living tissues to certain forms of vegetal parasites, are of more than historic interest.

As bearing upon the various types of microscopes then in use (1849). It is of interest to note in the last paper, he describes for the first time muscular striæ in the posterior cell, and later the anterior cell, of a new species of gregarina, determining its animality⁹ which

⁸ "A Flora and Fauna within Living Animals," Smithsonian Institution, 1851.

⁹ "Gregarina Dufox," *Proc. A. N. S.*, 1849. See also "Collected Researches in Helminthology and Parasitology," by Joseph Leidy, 1823-91, Smithsonian Institution, 1904.

had been previously denied by Creplin¹⁰ and von Seibold.¹¹

Leidy, in his monograph on the Gregarina, published this year, attributes the failure on the part of these investigators to note the presence of muscular striæ, to the inferiority of the microscopes used on the continent of Europe compared with those in use in England and America (1849).

Finally in a third paper published February, 1850, *Philadelphia Proc. Acad. Nat. Sci.*, Leidy writes it was now eighteen months since he had sought for Entophyta within living animals, having been previously impressed with the belief of their existence upon reflecting upon the essential conditions of life. Four months since he exhibited to the Academy numerous drawings and specimens of Entophyta obtained from living animals; he now exhibited others.

The essential conditions of life are five in number, viz., a germ, nutritive matter, air, water and heat. The four latter undoubtedly exist in the interior of living animals, animal or entozoa germs also are well known to exist, and it was rendered extremely probable that vegetable germs would also exist, and with them all the conditions necessary to vegetable growth. Plants have been very frequently observed growing upon the exterior of animals, and less frequently upon the interior, most usually upon diseased surfaces, but the growth of such parasites had not been pointed out as a normal and common condition as in the case of entozoa.

He next reviewed the theory of generation. He inclines to the opinion that sexual elements are absolutely necessary for the perpetuation of germs. He considered the *alternation* of generation in certain animals no objection to the law, for after successive developments an admixture of sexual elements is observed to be necessary. The reproduction among Cryptogamia may probably often exhibit phenomena analogous to the *alternation* of generation of animals, but universally he thinks it will be discovered that a true sexual

mixture takes place in every species of these plants at some period of their life. According to the observations of Schimper, it is necessary among the mosses. From an observation made by Klencke upon a fungus which grew upon a diseased surface, Dr. Leidy thinks that sexual admixture would be discovered to take place in the mycelium. In numerous instances it had been observed among the Algæ. He stated he thought he had noticed the process in *Achyla prolifera*, and gave a description of the phenomena. He finally considers that science is on the eve of demonstrating the existence of a law "that an admixture of sexual elements is necessary for the perpetuation of specific life germs."

He then exhibited numerous elaborate drawings of new entophyta observed growing in the ventriculus of *Passalus cornutus*, a remarkable one growing in a honey-like liquid in the pro-ventriculus of the larva of *Arctia Isabella*, another from *Acheta abbreviata*, etc. He remarked that when such plants were found in animals they were usually very abundant.

Dr. Leidy then stated that *very slight modifications in the five essential conditions of life were sufficient to produce the vast variety of living beings upon the globe*. As an instance, he mentioned he had lying upon his table a saucer with a cork bottom, in which lay a partially dissected *Passalus cornutus* half immersed in water. Two days afterwards he noticed on the part of the insect above the water a quantity of *Mucor mucedo* growing, and from the part within the water numerous fine, stiff filaments, which upon examination proved to be *Achyla prolifera*; upon the cork around the insect grew a third genus, consisting of fine cottony filaments, which were articulated, of which he exhibited a drawing; and upon the insect at the surface of the water, but not within the latter, grew a fourth genus, of which he also exhibited a drawing.

He also stated that he had had the good fortune of observing in a single morning all the stages of development of *Achyla prolifera* growing from some individuals of *Ascarides* which had been lying in a dish of water for a few days.

¹⁰ Wiegmann's *Archiv*, 1846, 1 Band, S. 157.

¹¹ Wiegmann's *Archiv*, 1838, 2 Band, S. 308.

In reply to some remarks made by members, Dr. Leidy said he could not admit the doctrine of spontaneous generation,¹² but rather modifications in the essential conditions of life favorable to the development of different and always preexisting germs derived from a parent.

It is but natural that these researches should lead to a discussion of the hypothesis of spontaneous generation and the origin of species. On these further researches I should like to dwell, bearing, as they do, upon the germ theory, but I fear I have already taxed your patience, so I must forbear.

From these published researches, in any historical review of the history of bacteriology, the usual accepted date of Davaine's designation of the vegetal nature of these organisms, *Vibrio*, *Spirillum*, 1859, should be moved back at least another decade to 1849.

JOSEPH LEIDY, JR.

SOUTH AFRICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE¹

THE twelfth annual session of the South African Association for the Advancement of Science was held in Kimberley, Cape Province, during the week commencing Monday, July 6, under the presidency of Professor R. Marloth. There was the usual round of festivities and of visits to places of scientific or historic interest. The papers read numbered between forty and fifty. Dr. A. Ogg, professor of physics at Rhodes University College, Grahamstown, in his presidential address to Section A, dealt with some of the ideas in physical science which are under discussion at the present time in the light of recent research, and sought to bring under review some of our fundamental notions or principles, having regard to the fact that what mathematicians and

physicists have long considered well established is now being uprooted and replaced by non-Newtonian mechanics based on the principle of relativity. In Section B the presidential address was given by Professor G. H. Stanley, of the Transvaal School of Mines and Technology, whose subject was "A Decade of Metallurgical Progress on the Witwatersrand." The greatest advances during the last ten years, he said, were in improving methods of carrying out the various stages of the extraction processes, the essentials remaining unchanged. In Section C, comprising the biological sciences and agriculture, the presidential address of Professor George Potts, of Grey University College, Bloemfontein, dealt with rural education. An evening discourse was delivered in the Kimberley City Hall by Professor E. H. L. Schwarz, on the Kimberley diamond pipes, the history of their discovery, and their relation to other South African volcanic vents. This lecture, like Professor Marloth's address as president of the association was illustrated by many lantern slides. The numerous slides exhibited by Professor Marloth were all hand colored, and constituted the most excellent collection representative of South African indigenous flora ever exhibited. At the conclusion of the president's address, Dr. Crawford, the association's senior vice-president, handed to him the South Africa medal (instituted by the British Association in 1905 in commemoration of its visit to South Africa during that year) and grant of £50 which had been conferred upon him in recognition of his eminent services to botanical science in South Africa during the last thirty years.

PACIFIC FISHERIES SOCIETY

ON March 11 a meeting of those interested in the upbuilding and perpetuating of the great fisheries of the Pacific slope was held in Seattle, Wash., and it was decided to form a temporary organization of a society to be known as the Pacific Fisheries Society, and to hold a meeting later in the year for the pur-

¹² For experiments in connection with the theory of spontaneous generation, see "Flora and Fauna within Living Animals," Smithsonian Institution, 1851, *et al.*, published lectures before students of medical department, University of Pennsylvania, 1858 and 1859.

¹ Abridged from a report in *Nature*.

pose of making the organization a permanent one. The following officers were elected: *President*, Carl Westerfeld, member California Fish and Game Commission, San Francisco, Cal.; *Vice-president*, Henry O'Malley, Pacific Coast Supt. of Hatcheries for U. S. Bureau of Fisheries, Oregon City, Oregon; *Vice-president*, Professor Trevor Kincaid, head of Department of Zoology, University of Washington, Seattle, Wash.; *Secretary*, John N. Cobb, editor *Pacific Fisherman*, Seattle, Wash., and *Treasurer*, Russell Palmer, Seattle, Wash.

The first annual meeting was held at the University of Washington, Seattle, on June 10-12, when the following papers were read:

Policy of the U. S. Bureau of Fisheries with respect to the Pacific Fisheries, by Dr. H. M. Smith, Commissioner of Fisheries.

Establishment of a Fishery School at the University of Washington, by Professor Trevor Kincaid.

Some Neglected Fishery Resources of the Pacific Coast, by John N. Cobb.

Angling and Netting; the Conservation of the Marine Fishes of Southern California, by Dr. Charles F. Holder.

The Nanaimo, British Columbia, Biological Laboratory, by C. McLean Fraser, Director.

Hybridization of Salmon, by Professor Victor E. Smith.

Rearing and Feeding Salmon Fry. Separate papers by Henry O'Malley, of the U. S. Bureau of Fisheries; W. H. Shebley, California Supt. of Hatcheries; R. E. Clanton, Oregon Supt. of Hatcheries; Stephen Butts, Supt. Willapa State Hatchery, Lebam, Wash., and L. M. Rice, Supt. Chehalis, Wash., Hatchery.

The society decided to retain for another year the officers elected at the March meeting, and in addition the following to serve as an executive committee: Dr. Barton W. Evermann, Director Museum California Academy of Sciences, San Francisco, Cal.; C. McLean Fraser, Director Biological Laboratory, Nanaimo, British Columbia; Dr. Charles F. Holder, Pasadena, California; Leslie H. Darwin, Washington Fish and Game Commissioner, Seattle, Wash.; M. J. Kinney, member Oregon Fish and Game Commission, Portland, Oregon; Ward T. Bower, Pacific

Coast Agent U. S. Bureau of Fisheries, Seattle, Wash., and M. D. Baldwin, Esq., member Montana Fish and Game Commission, Kalispell, Montana.

The next annual meeting will be held in San Francisco in 1916, the date to be fixed later.

The society will publish its annual proceedings.

JOHN N. COBB,
Secretary

THE AMERICAN CHEMICAL SOCIETY

As was announced in last week's issue of SCIENCE the American Chemical Society is unable to hold the meeting which had been planned for Montreal in September. The conditions are explained in the following letter addressed to Dr. Charles L. Parsons, secretary of the society, by Professor R. F. Ruttan, chairman of the Montreal committee:

The declaration of war between Germany and England found me at Metis Beach, 500 miles down the St. Lawrence, playing golf with a feeling of relief that our organization for the meeting was so complete.

My first wire to you was mis-sent by a habitant operator, who did not think the order of initials was of any importance. I am sorry for the delay. I took the first train back to Montreal, arriving this morning, and wired you.

We had a meeting of all the executive committee in town this afternoon, and with profound regret, fully realizing what it meant to you and the society, decided that the meeting could not be made to go in British territory this autumn. I wired you at once.

"Canada is sending the first contingent of 20,000 very soon and a second and third will follow.

"Montrealers feel that we are at war with Germany and Austria, and are behaving as if the enemy were threatening us.

"The harbor, canals, etc., are under martial law. The excursions were off, as the company cancelled our contract, for the steamers for the rapids and harbor.

"No German member of the society would naturally come to British soil and all with German names would be questioned at the boundary. Many are even now turned back. We felt that the ex-

clusion of so many prominent members of the society was a high price to pay for a meeting here.

"Any foreigners would be subjected to disagreeable formalities and conditions on coming here just now.

"It would be impossible to attract to the convention the slightest public interest in Montreal, outside a few dozen chemists. No one would come to the conversazione or the garden parties we had arranged, and while there would surely be the feeling of good fellowship among ourselves, it would be overshadowed by the tragic war we are in at present."

It is sad to look over the wreck of our hopes of a big and successful meeting.

Everything was organized and under way even to rehearsing for the smoker. The toastmaster and speakers for the banquet, the chemical and other scientific "stunts" for the conversazione were arranged, the hall for the exhibits prepared, which, by the way, would have been of exceptional interest. We feel very sad about it all to-day I assure you.

The principal, vice-principal and Sir Wm. Osler, who had promised to speak at the banquet, are in Europe, as well as many of our staff. Their return is uncertain. Everything was against the meeting and only our desire to give you the hand of good fellowship and the advanced state of the preparations made us hesitate at all about calling everything off.

I hope you appreciate our situation and that we have your sympathy.

I came up this morning feeling sure the meeting would go, but have been convinced it could not be made more than an apology for a convention, which it would be a waste of time to attend.

When things settle down again we will once more extend you an invitation, and hope you will do us the honor of accepting it.

On receipt of this letter, President Richards of course determined at once to call off the meeting. The almost unanimous opinion of the officers of the society is that it will be impossible to arrange for a successful meeting early in the fall and that business conditions throughout the country render it improbable that it would be advisable to have a meeting later in the year. The present outlook is that the next meeting of the American Chemical Society will be in New Orleans, April 1 to 3, 1915.

SCIENTIFIC NOTES AND NEWS

MR. ROOSEVELT has arranged to give to members of the American Museum of Natural History in the fall the first presentation of the zoological results of his recent expedition to South America. The zoological collections which, through the generosity of Mr. Roosevelt, the museum has received from the Roosevelt expedition to South America, amount to twenty-five hundred birds and four hundred and fifty mammals.

THE Bissett-Hawkins memorial medal of the Royal College of Physicians of London has been awarded to Sir Ronald Ross, for his work on malaria.

At a meeting of the Royal Society of Edinburgh, held on July 7, Dr. W. S. Bruce was presented with the Neill Prize, in recognition of the scientific results of his Arctic and Antarctic explorations.

DR. ALEXANDER VON BRILL, professor of mathematics at Tübingen, has been given a doctorate of engineering by the Technological School at Munich, on the occasion of the fiftieth anniversary of his doctorate.

DR. PAUL KROEBER, of Leipzig, has received a prize of 5,000 Marks from the Berlin Academy of Sciences for his work on the theory of functions.

DR. MAIRET, professor of mental and nervous diseases at Montpellier, has been elected a national associate of the Paris Academy of Medicine. He has been national correspondent in the section of pathologic medicine since 1894.

THE second annual meeting of the Indian Science Congress is to be held, under the auspices of the Asiatic Society of Bengal, in Madras, on January 14-16 next, under the presidency of Surgeon-General Bannerman.

DR. BEVERLY T. GALLOWAY, lately assistant secretary of the Department of Agriculture, took up his duties as director of the New York State College of Agriculture, Cornell University, on August first.

DR. LEWIS A. SEXTON, resident physician at Willard Parker Hospital, New York, has accepted the position of superintendent of the

Johns Hopkins University Hospital, Baltimore.

DR. C. O. TOWNSEND, formerly in charge of sugar beet investigations for the U. S. Department of Agriculture but more recently in commercial sugar beet work at Garden City, Kansas, has returned to Washington and is again in charge of sugar beet investigation for the Federal Department of Agriculture.

MR. HARLAN I. SMITH, archeologist of the Geological Survey, Canada, is exploring in the shell-heaps of Merigomish, Nova Scotia. Mr. W. B. Nickerson is continuing explorations in the mounds, earthworks and village sites of southwestern Manitoba, and Mr. W. J. Wintemberg is exploring a section of country between Prescott and Peterborough, Ontario, for a site of a culture different from that of the Roebuck site which he excavated in 1912.

MR. F. M. ANDERSON, curator, and Mr. Bruce Martin, assistant curator of the department of invertebrate paleontology of the California Academy of Sciences, with two assistants, have recently left for South America where they are engaged in making oil investigations for an oil company. Their field of investigation is in the United States of Colombia at Lorica which is midway between the Magdalena and Atrato rivers. They have already made extensive collections of invertebrate fossils in the tertiary strata of that region and they expect to make still larger collections incidental to their work during the next year. These collections will come to the California Academy of Sciences.

DR. C. W. HAYES, who resigned the office of chief geologist in the United States Geological Survey in 1911 to take a position as vice-president and general manager of the Mexican Eagle Oil Company, with headquarters at Tampico, has left Mexico for England. He retains his connection with the company as first vice-president, but will no longer act as general manager. He will be occupied chiefly as geological adviser to S. Pearson & Son, Ltd., of which Lord Cowdray is the head, in connection with the operations of that firm in various parts of the world.

MR. OLE OLSEN has offered to place at the disposal of Mr. Knud Rasmussen funds sufficient for the fitting out of a north polar expedition. Mr. Rasmussen has already traveled much in Greenland and has made studies of the Eskimo. The proposed expedition would take provisions for two years and would include a scientific staff. A base camp would be set up at Cape York, Greenland, and the expedition would probably start in 1915.

THE new session of the medical faculty of the University of Manchester will be opened on October 8 by an address by Professor E. S. Reynolds on the industrial diseases of Greater Manchester.

PROFESSOR ARTHUR CARLETON TROWBRIDGE, of the State University of Iowa, gave an illustrated lecture at the University of Chicago on August 13 on "Some Mountains of the United States and Their Inhabitants," and Henry Oldys, formerly of the United States Biological Survey, lectured on August 20 and 21 on "Bird Protection and Bird Music" and "Birds at the National Capital."

A STATUE of Captain Cook, by Sir Thomas Brock, R.A., has been erected by public subscription in London, on the Mall side of the Admiralty Arch, at the end of the Processional Road, and was unveiled on July 7 by Prince Arthur of Connaught.

DR. EDOUARD REYER, professor of geology at Vienna, has died at the age of fifty-six years.

IT is reported that in future the distribution of the Nobel prize will take place on June 1 instead of in December, as hitherto. The next distribution has been fixed for June 1, 1915.

ANNOUNCEMENT is made that the International Ophthalmological Congress, which was to have been held at St. Petersburg in August, has been postponed, and the same course will doubtless be taken for all the international congresses which had planned to meet in Europe this year.

IT is stated in *Nature* that the whole of the new buildings of the University of Birmingham at Edgbaston have been taken over by the war office, and now form the first southern

general hospital. Certain structural alterations are being carried out with a view of making the hospital as efficient as possible.

OUTSIDE of Germany there is no known commercial supply of potash salts. If the German supplies are cut off during the European war, the agricultural world must either go without potash salts after the meager supply now on hand is exhausted or bestir itself to find another adequate source of supply. Already many inquiries regarding potash have been addressed to the United States Geological Survey, and the fertilizer journals report that small quantities of spot material are changing hands at sharp premiums. The situation is undoubtedly more acute than it was a few years ago, when national interest was first awakened to the fact that the United States is entirely dependent on Germany for this important class of fertilizer materials. Potash salts are employed in many industries other than the fertilizer industry. A large amount is used in glass and soap making and in the manufacture of a number of chemical products. These include potassium hydrate, or caustic potash, and the carbonate and bicarbonate of potash, used principally in glass and soap making; the potash alums; cyanides, including potassium cyanide, potassium ferro-cyanide, and potassium ferri-cyanide; various potash bleaching chemicals, dye stuffs, explosives containing potash nitrate, and a long list of general chemicals. The imports of potash salts, listed as such in the reports of the Bureau of Foreign and Domestic Commerce, include the carbonate, cyanide, chloride, nitrate and sulphate, caustic potash, and other potash compounds. The importation of the above salts in round numbers the last three years has averaged 635,000,000 pounds in quantity and \$11,000,000 in value. These figures, however, represent only a part of the potash salts entering the United States as they do not include the imports of kainite and manure salts which are used in fertilizers. The quantity of this class of materials imported for consumption in the United States during the last three years has averaged about 700,000 tons valued at \$4,300,000 annually. Thus it is apparent that the

annual importations of potash salts exceed \$15,000,000.

THE outbreak of the European war has caused the New York price of tin to rise to 65 cents a pound, although in the latter part of July tin was sold as low as 30.5 cents a pound. None of the European countries make a production which would greatly affect market values, and the disturbance of price is due mostly to the insecurity of ocean freights. The known American tin deposits are small, and production from them will probably not be much affected by the exceedingly high prices if these are temporary. However, the operators now working tin deposits may reap a profit if they can market their ores before the drop in prices that is sure to come. The benefit which it seems possible to get out of the present situation is in the establishment of a tin smelter in the United States in which to smelt Bolivian tin ores and such small lots of American ore as are produced. At present between 30,000 and 40,000 tons of tin concentrates carrying more than 20,000 tons of metallic tin are shipped each year from Bolivia to Europe for smelting. The United States would absorb the tin smelted from this ore easily, and it has been demonstrated that there are no difficulties in the smelting of the Bolivian ores that American metallurgists can not meet. Owing to the lack of European freighters, Bolivian ores will now be seeking a market, and, providing that ships can be found to carry the ore, this will be the opportunity for Americans to begin purchasing the ores that have heretofore gone to Europe. A few years ago a smelter was established at Bayonne, N. J., in which to smelt Malayan tin ores, but when it became known the English government placed a high export duty on Malayan tin ores not going to some part of the British empire. Such a thing could not happen in Bolivia, and to some extent, at any rate, the smelting of Bolivian and other ores in this country would relieve American consumers from the speculative profits of the London market.

ANTIMONY is ordinarily one of the cheaper metals, selling at one and a half times to twice

the price of zinc, but since the outbreak of the European war it has reached more than 20 cents a pound, a price higher than that of aluminum. During the six years from 1908 to 1913, inclusive, the price of Cookson's antimony ranged from 7.45 to 10.31 cents a pound, and the yearly averages ranged from 8.24 to 8.58 cents a pound. Much of the time during the present year the price has been still lower, and toward the end of July it was quoted as 7 to 7.10 cents. Other brands have ranged from 0.25 to 1.25 cents lower. As has been pointed out in the United States Geological Survey's reports, at these prices antimony ores can not be worked profitably under the high labor costs prevailing in the mining regions of the United States unless the deposits are very large and advantageously situated. No deposits of antimony ores have been found in the United States which entirely fulfill these conditions, and as a result practically all the antimony metal used here is imported from European smelters, mostly from England. The ores for these smelters come largely from China, Mexico, France and Austria. So long as the war exists and especially so long as sea traffic is disturbed, the production will be curtailed and prices raised, for the use of antimony in type metals and especially in bearing metals is fixed and will continue. Other uses, such as the making of coffin trimmings, which consume a surprisingly large quantity of antimony and from which there is no secondary recovery, might conceivably turn to aluminum or other metals as substitutes. In the United States deposits of stibnite (antimony sulphide) near Gilham, Ark.; Battle Mountain, Lovelocks and Austin, Nev.; Burke and Kingston, Idaho; Tonasket, Okanogan County, Wash.; Graniteville and San Emigdio Canyon, Cal.; Antimony, Utah; Red Bridge, Ore., and other places are potentially productive in times of prices as high as those now prevailing. A greater benefit than the temporary operation of the mines would probably accrue to this country from the establishment of smelters which would import and smelt Chinese, South American, Canadian and Mexican antimony ores. At present the only reg-

ular antimony smelting in this country is done by a smelter which is said to be a branch of an English smelter.

UNIVERSITY AND EDUCATIONAL NEWS

PROFESSOR ALEXANDER KÖNIG, of Bonn, has presented to the University at Bonn the zoological museum and laboratory which he has erected, to be called the Alexander König Museum. The collections are valued at a million Marks.

It may be noted that it was planned to open the new university at Frankfort-on-the-Main October 18 in the presence of the German emperor.

THE Royal School of Mines in Freiburg, Saxony, said to be the oldest school of technology, will celebrate the hundred and fiftieth anniversary of its foundation in July, 1915.

AT Syracuse University, college of medicine, a course in pathology was offered during the summer. The course opened on June 15, and continued for six weeks. It was open to both graduates and undergraduates in medicine. There were daily sessions covering the entire day.

PROFESSOR T. G. ROGERS, of the New Mexico Normal School, of Silver City, has been elected professor of mathematics and assistant dean of the Normal University of New Mexico, at East Las Vegas.

DR. O. C. GRUNER, assistant professor of pathology at McGill University, has resigned and returned to England.

DR. LUDWIG BÜRCHNER, of Munich, has been called to the chair of geography at the University of Athens.

DISCUSSION AND CORRESPONDENCE

A NOTE ON DISTINCTION OF THE SEXES IN PHRYNOSOMA

A SURPRISINGLY small amount of knowledge concerning the embryology and development of the Iguanidæ has been collected. One reason for this is the fact that, for most forms, there is no reliable method of distinguishing the sexes by external characters. This is par-

ticularly true in the case of the familiar, but little studied, "horned toad," *Phrynosoma cornutum*, and undoubtedly many "pairs" which have been shipped north by well meaning collectors have been of the same sex.

In making a study of the stomach contents of *Phrynosomas*, I have had occasion to open some two hundred specimens, trying always to find some connection between external characters and sex. The problem very quickly was solved; and I can affirm, that for this region at least, and during the spring months, the crescent markings on the back of the female are much brighter yellow than those of the male. The difference is very marked, and little or no practise is required to enable one to distinguish the sexes, even without comparison of specimens.

W. M. WINTON

TEXAS CHRISTIAN UNIVERSITY,
FORT WORTH, TEXAS

CAHOKIA OR MONKS MOUND NOT OF ARTIFICIAL ORIGIN

A STUDY of the materials composing the so-called Monks or Cahokia Mound, in Madison county, Ill., establishes, beyond doubt, that it is not of artificial origin, as has been so generally held but that it is a remnant remaining after the erosion of the alluvial deposits, which at one time filled the valley of the Mississippi, in the locality known as the "Great American Bottoms."

A. R. CROOK

SPRINGFIELD, ILL.

SCIENTIFIC BOOKS

Geology of the Yang-tze Valley (China). By YAMAJIRO ISHII. Bulletin of the Imperial Geological Survey of Japan, Vol. 23, No. 2, Tokyo, 1913, pp. 19 + 157.

There are but few inhabited and easily accessible parts of the globe about which there is a smaller fund of geological knowledge than China. For that reason it is gratifying to note that papers on Chinese geology are appearing with increasing frequency. On the other hand, it is regrettable that some of these do not pos-

sess either the practical utility or the scientific accuracy that is always needed.

Since it is printed in the Japanese language and characters, Mr. Ishii's paper on the Yang-tze Valley will be of little use to nearly all geologists outside of Japan and China. This applies not only to the text, but also to the titles of maps and diagrams. Although there may be some compelling reasons unknown to the reviewer, such as popular demands in Japan, it would be hard to defend on general grounds, the printing of technical scientific papers in any language which is not in more or less general use in the scientific world. Only a geologist can read a technical geologic paper with full understanding and appreciation. Nearly all educated Japanese and Chinese read English, if not also French or German, so that even a paper intended largely for local use in Japan would be quite as intelligible to its readers if presented in one of the more important European languages and it would at the same time be available for foreign students in general. A popular summary in Japanese might be appended for the edification of the few who read only the mother tongue. It is greatly to be hoped that the future tendency in Japan will be away from the practise exemplified in this bulletin.

In the English summary of 19 pages at the beginning of the bulletin, there is an interesting account of the origin of the name Yang-tze-Kiang. This is followed by paragraphs on "Hydrography," and "Mountains and Plains." Under the heading of "Geology," the following table of stratigraphic divisions is given: (a) Quaternary, (b) Red Sandstone formation, (c) Coal-bearing Sandstone formation, (d) Great Limestone formation, (e) Sinic or Metamorphic formation, (f) Gneiss formation, (g) Plutonic rocks, (h) Volcanic rocks. The reviewer is obliged to agree with the author's admission (on page 16) that "our classification of the strata in Yang-tze Valley into the Quaternary, red-sandstone formation, coal-bearing formation, etc., as given above, is not the proper method of classification, because the geological age of each member is so indefinite that one formation may represent older Paleo-

zoic and middle Mesozoic." He would suggest that a much better classification could have been devised by a more careful study of the reports of earlier geological expeditions in China, which have evidently furnished a large proportion of the material embodied in Mr. Ishii's paper.

The author regards the well-known red beds of Sze-chwan as either Cretaceous or Tertiary and believes that they were deposited in a salt lake or inland sea. The "Coal-bearing Sandstone Formation" appears to include rocks of widely different age, such as the Permo-Carboniferous coal-bearing beds described by the Carnegie Expedition of 1903-04 and the Rhaetic-Lias of Richthofen and Loczy. There is probably little more than a lithologic resemblance between these two series. In his description of the Paleozoic limestones, the author adds but little to that which is already known and, on the other hand, confuses much that has already been published. He refers to the Cambro-Ordovician limestones described by the Carnegie Institution as a "metamorphic limestone" and implies that its thickness can not be measured. These are surprising errors in view of the reported fact that along the Yang-tze gorges the limestone is almost entirely unaltered, fossiliferous, and only gently folded; and the thickness was measured ten years ago as well as the very small amount of time devoted to the act would permit. The very fact that a generous collection of well-preserved fossils has already been taken from these rocks is sufficient evidence that the alteration of these strata is not everywhere severe. One finds no mention in these pages of the interesting change in metamorphism of the Paleozoic rocks from the Yang-tze River itself, where the beds are merely consolidated, northward into southern Shensi, where they are schistose. Nor are the Cambrian glacial beds of Nan-tou, which have attracted wide attention among geologists, given even passing mention. Perhaps these points are discussed in that portion of the paper which is a sealed book to the occidental reader.

The author's interpretation of the geologic structure and history of central China will

hardly commend itself to other students of the region. He apparently regards the Yang-tse basin as originally a great depression in a granitic foundation, enclosing a great lake or inland sea. This was gradually filled by successive layers of Paleozoic and Mesozoic rocks so that it dwindled in Cretaceous or Tertiary times to small remnants in the neighborhood of central Sze-chwan and the Tung-ting lake. Some time after the Paleozoic, the mountain ranges were produced by horizontal pressure which developed the folds and many minor basins. The author appears to hold the opinion that the last of the inland seas overflowed their rims and that these outlet rivers cut the magnificent gorges of the Yang-tze and its tributaries. Whether or not the author has given any consideration to the other published explanations of the phenomena must remain unknown to a reviewer who is unable to read the Japanese text.

A perusal of the English summary suggests that the material for the bulletin has been derived largely from a somewhat hasty or ill-considered examination of the reports of foreign geologists who have previously made explorations in China, interpreted in the light of the author's own field work. That the author was adequately prepared for his important task by sound and thorough training in school and field under competent guidance is not indicated by the available results. One of the most commendable characteristics of the paper is, nevertheless, the distinction which is generally made between inferences and facts,—a virtue which not a few occidental writers on geology might imitate to advantage. It is suggested that a more appropriate title for Mr. Ishii's paper would be "A Preliminary Geologic Sketch of the Geology of the Yang-tze Valley." Before the geologic features of so great and complex an area will have been described with even comparative thoroughness, the product will require many large volumes rather than a small pamphlet.

ELIOT BLACKWELDER

UNIVERSITY OF WISCONSIN

Die Oekologie der Pflanzen. By DR. OSCAR DRUDE. Band 50, Die Wissenschaft Sammlung von "Eingeldarstellungen aus den Gebieten der Naturwissenschaft und der von Friedr. Vieweg & Sohn. 1913. Pp. Technik. Braunschweig, Druck und Verlag viii + 308, with 80 figures in the text.

Not since the publication of Warming's "Oecology of Plants" in English in 1909 has a general work on the ecology of plants appeared. Professor Drude comes well-equipped for the presentation of the subject by years of study and travel in Germany, Great Britain and the United States. A student of Grisebach's, one of the earliest and greatest of plant geographers, Dr. Drude has seen the rise and progress of plant geography and ecology, and his first chapter on physiognomic growth forms of plants in which a historic review of ecology is given is written from personal acquaintance with the prime movers in the new department of botanic science. The first one hundred pages deal with the physiognomic life forms of plants. Beginning with page 31, a classification of these forms is given with numerous figures and reference to illustrative plants. Some of the groups considered are Monocotyledonous Crown Trees, Tree Ferns and Cycads, Dicotyledonous Woody Lianes, Grass Trees, Dicotyledonous Stem Succulents, Perennial Grasses, Dicotyledonous Cushion Plants, Geophilous Bulbous Plants, Saprophytes and Parasites. Altogether Drude recognized 54 growth forms, grouped under the heads of Aërophytes, Aquatic Plants and Cellular Plants (mosses and thallophytes), etc. Following the general consideration of each group, notes are given for purposes of further study and cross reference and bibliographic details are cited. Illumination illustrations and additions end this instructive chapter.

The second chapter deals with climatic influences, periodicity of vegetation and leaf characters. The topics treated in this chapter describe the physiognomic effect and organization of the leaf and the physiologic questions of plant nutrition. Here the author deals with the duration of the leaf, bud formation and protection, light and leaves, transpiration,

etc. Under climatic periodicity, the author gives a geographic division of the climatic zones, recognizing 18 climatic groups. Phenology and other problems of climatic influence are considered in detail in this chapter.

The third chapter is concerned with physiographic ecology. The ecologist must deal with the difficult problem of why species unite into certain communities and why they have the physiognomy which they possess? The author treats of the edaphic influences of soil, ground water, bacteriologic soil content and the influence of lime and acids. He quotes Jaccard's law on the distribution of species in the alpine meadows and pastures, and deals with the much discussed question of association and formation. The last section of this chapter deals with thirteen vegetation types, viz., hydrophytes, helophytes, oxylophytes, halophytes, lithophytes, psychrophytes, psamphytes, eremophytes, chersophytes, psilophytes, sclerophytes, conifers and mesophytes.

The fourth chapter, and last one, is devoted to matters of evolutionary interest and is headed ecologic epharmony and phylogeny. In several sections, phylogeny and growth forms, eurychory and stenochory, correlation, epharmony, mutation and heredity are considered. Additional notes and a bibliography complete the volume.

Altogether, ecologists, the world over, will be indebted to Professor Drude for a lucid exposition of the important principles of that department of botanic science denominated ecology. He has presented much that is entirely new, and he has made over into a different form much that is old. The whole book shows a thorough grasp of the entire subject of plant ecology, which the author has been able to digest and assimilate and present in an attractive and useful form to the student world. The figures are good and many of them new, representing typic species, some of them grown in the Dresden Botanic Garden.

JOHN W. HARSHBERGER

UNIVERSITY OF PENNSYLVANIA

A Treatise on Quantitative Inorganic Analysis. By J. W. MELLOR, D.Sc. Philadelphia, J. B. Lippincott & Co.

This excellent work is Volume 1 of a treatise in the ceramic and silicate industries by the same author. The processes used are those used in the testing department of the County Pottery Laboratory, Staffordshire, for the analysis of clay, bricks, glazes, enamels, refractories, and for the coloring materials and minerals used in ceramics.

The book is divided into five parts with an historical introduction of ten pages.

Part I., containing 140 pages, takes up rather exhaustively the following chapters: I. Weighing, 25 pages; II. The Measurement of Volumes, 17 pages; III. Volumetric Analysis, 37 pages; IV. Colorimetry and Turbidity, 5 pages; V. Filtration and Washing, 23 pages; VI. Heating and Drying, 10 pages; VII. Pulverization and Grinding, 7 pages; VIII. Sampling, 14 pages; IX. The Reagents, 11 pages.

Part II. containing 98 pages takes up carefully and in detail the analyses of clays and other silicates. The accuracy obtainable is illustrated by tables giving the results of actual analyses of silicates showing the variations to be expected for each determination. The methods used are practically those used by the U. S. Geological Survey somewhat shortened.

Part III., containing 121 pages, takes up the analysis of glass, glazes, enamels and colors, including the determination of arsenic, antimony, tin, lead, bismuth, mercury, copper, cadmium, zinc, manganese, cobalt and nickel.

Part IV., 128 pages, describes special methods for the determination of the following: molybdenum, tungsten, niobium, tantalum, gold, selenium, aluminum, beryllum, iron, chromium, vanadium, uranium, zirconium, thorium, the rare earths, barium, strontium, calcium, magnesium and the alkalies.

Part V., containing 111 pages, describes special methods for the acids and non-metals, carbon, boron, oxide, water, phosphorus, sulfur, the halogens, and the rational analysis of clays.

Finally the Appendix contains 55 pages of analytical tables, etc.

This work is just what its title indicates, "A Treatise on Quantitative Inorganic Analy-

sis," written more especially with the needs of the ceramic chemist in view. It is profusely illustrated with photographs, drawings and graphs, and the bibliography given in the footnotes is quite complete.

The methods given are perhaps somewhat unnecessarily long for the technical chemist, but this is on the safe side and the chemist can shorten the methods to suit himself. Dr. Mellor has left out gas and fuel analyses on the ground that there are so many books specializing in these subjects.

The book is a very helpful addition to the library of the analytical chemist, particularly because it keeps in view always the analysis of the kind of things the chemist has actually to analyze and not merely pure salts. It will be invaluable to the ceramic chemist.

Dr. Mellor is to be congratulated on the completion of this work.

D. J. DEMOREST

THE COLLEGE CURRICULUM

PRESIDENT MEIKLEJOHN, of Amherst College, in his recent annual report, makes some interesting contributions to the discussion of the college curriculum. In the first place, he shows it to be an unfounded rumor that Amherst has become distinctly a "classical" school, to the neglect of the sciences. Dean Ferry's statistics of student registration, published last year in *SCIENCE*, give Amherst a median position among the New England colleges, both in science and in the classics, as well as in English and other modern languages, and a low position only in the "humanistic sciences," including history, economics and philosophy. It is true that Amherst has abandoned the B.S. degree, but this was done partly because that degree attracted a lower grade of students and was regarded as inferior to the Arts degree and easier to obtain, and partly for the purpose of simplifying the mechanism of a prescribed curriculum, to which policy Amherst is now committed. For the last few years, its curriculum has been largely prescribed and has demanded much concentration upon "majors." The plan has been found defective in one respect, since

the absence of the humanistic sciences from the freshman and sophomore years, along with the requirement of continuing courses, has operated to keep students out of these subjects. This defect has now been remedied by introducing philosophy into the sophomore year, and a course on "social and economic institutions" into the freshman year.

The curriculum now adopted is to be regarded as but a station on the road to a course almost wholly prescribed, and organized about one great central purpose, that, namely, of initiating the student into an understanding of human experience and the moral and intellectual problems of the times. President Meiklejohn offers a sketch of the ideal college course, as he sees it coming—merely a sketch, confessedly, which will need correction as the result of abundant discussion. The plan certainly is radical. Of the four-year course, 66 per cent. is prescribed, and half of the remainder must be devoted to the senior "major," which is itself to be a continuation of some junior study. The prescribed work is divided as follows: 15 per cent. (of the whole curriculum) to mathematics and natural science, 15 per cent. to literature and 36 per cent. to the humanistic sciences. In favor of this plan, there is this at least to be said, that it follows the trend of the times. While discussion has been raging over the relative values of natural science and the classics, the student body, where free, has attached itself to modern literature and especially to the humanistic sciences. At Harvard, according to Dean Ferry's figures, 3 per cent. of student registration goes to the ancient languages, 25 per cent. to mathematics and science, 28 per cent. to modern literatures and 44 per cent. to "other subjects"; and Professor Hervey has found almost exactly the same proportions among elective subjects in Columbia College. The emphasis on the "other subjects," in President Meiklejohn's plan, may thus be taken as meeting a demand voiced by the students. The question may indeed be raised whether it is worth while, by faculty legislation, to require all students to do what the majority do of their own choice. Another query is sug-

gested by President Meiklejohn's objections to the elective system.

Under the elective scheme, no subject is essential. Why study physics hard when other students are getting an education without it? . . . The argument is bad but none the less convincing.

Under a required curriculum, the difficulty may be to keep the student in ignorance of the fact that the requirements are different at other colleges. It may also be difficult to explain to him why he should specialize on some one subject to the extent of devoting most of his senior year to it, when his classmate is acquiring a liberal education, presumably just as good, without specialization in this particular direction. If the student is genuinely in love with his subject, well and good—or if he sees a vocational value in it; but vocational values, we are assured, are to be left entirely aside from the curriculum of a liberal college.

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SPECIAL ARTICLES

ON SOME NON-SPECIFIC FACTORS FOR THE ENTRANCE OF THE SPERMATOZOON INTO THE EGG

1. While formerly fertilization was considered a single process which could be adequately described by the entrance of the spermatozoon into the egg or the fusion of the egg nucleus with the sperm nucleus, we know now, through the methods of experimental biology, that fertilization consists of at least three different groups of phenomena. These are, first, the transmission of paternal characters through the spermatozoon. This process is obviously a function of the chromosomes. Second, the causation of development of the egg, which is apparently independent of the chromosomes since the experiments on artificial parthenogenesis have shown that it can be induced by certain non specific agencies. The causation of development is a complicated process since it requires at least two agencies, one inducing an alteration of the surface of the egg (which sets the chemical processes underlying development in action), and the

other a corrective agency which guarantees a normal development.

The third group of factors involved in the process of fertilization is that determining the entrance of the spermatozoon into the egg. This note will deal with the latter problem.

2. We can undertake the analysis of the conditions necessary for the entrance of the spermatozoon into the egg from two different starting points, namely, by finding means for fertilizing the eggs with the sperm of distant species against which the egg is naturally immune; or by rendering the eggs immune against sperm of their own species. The former problem was solved for certain cases when the writer found that the eggs of the sea urchin (*Strongylocentrotus purpuratus*) which under normal conditions can not be fertilized by the sperm of the starfish or holothurians can be fertilized with such sperm if the sea-water is rendered more alkaline.

Last winter the writer found that an addition of calcium chloride to sea-water acts in the same way. In this case the above-mentioned hybridization can be brought about if little or no alkali is added to the sea-water. This suggested the idea that the forces determining the entrance of the spermatozoon into the egg depended upon the concentration of calcium and hydroxylions in the sea-water.

3. If this idea was correct it was to be expected that the elimination of these two substances might render the eggs which are naturally fertilized in normal sea-water immune against sperm of their own species. This was found to be the case. If eggs and sperm of *Arbacia* or *purpuratus* are freed from sea-water and put into a neutral mixture of $\text{NaCl} + \text{KCl}$ or $\text{NaCl} + \text{MgCl}_2$, or of $\text{NaCl} + \text{KCl} + \text{MgCl}_2$ (in the concentration and proportion in which these salts exist in the sea-water) no egg is fertilized. Yet it can be seen that sperm remains motile in these solutions for a long time (twenty-four hours or longer) and it can also be shown that newly fertilized eggs are able to segment in these solutions. If calcium chloride is added to these solutions fertilization will take place at once. The same is true when a trace of a base is

added to the mixture of $\text{NaCl} + \text{MgCl}_2$, or of $\text{NaCl} + \text{KCl} + \text{MgCl}_2$.

On the other hand, these eggs can be fertilized by sperm of their own species in neutral solutions containing calcium, namely $\text{NaCl} + \text{CaCl}_2$ or $\text{NaCl} + \text{KCl} + \text{CaCl}_2$, or $\text{NaCl} + \text{CaCl}_2 + \text{MgCl}_2$, or $\text{NaCl} + \text{KCl} + \text{MgCl}_2 + \text{CaCl}_2$. Similar results were obtained in regard to the fertilization of the eggs of an annelid (*Chaetopterus*) and a mollusk (*Cumingia*). It can, therefore, be stated that the entrance of a spermatozoon into an egg of its own or foreign species is determined by forces which are influenced by the concentration of calcium and hydroxylions in the solution, the difference in both cases being only in the concentration of these substances required.

4. The question arises which forces in the egg or spermatozoon are influenced by these two agencies. Since it seems tolerably certain that neither the strong base nor the calcium salts enter into the egg or the spermatozoon, the forces acted upon by these substances must be located at the surface of the egg or spermatozoon. There are only three kinds of forces that need be taken into consideration; namely, (1) surface tension, (2) adhesion between spermatozoon and egg surface, (3) cohesion or degree of fluidity of the surface of the egg. Experiments which the writer carried out last winter in Pacific Grove seem to indicate that the adhesion of the spermatozoon to other bodies is strongly influenced by both calcium and bases. The egg of the sea urchin is surrounded by a jelly which the spermatozoon must penetrate before it reaches the egg. If it should stick to the inner surface of the jelly it might still come in contact with the egg and might be able to impart to the surface of the egg, the membrane-forming substance; but through its adhesion to the jelly it might be prevented from entering the egg. The egg should, in consequence, be in the same condition as one in which the membrane formation has been induced by butyric acid but which has not been treated with the second corrective factor. It should show a membrane formation and a beginning of development, but should then perish.

The writer had observed in his earlier experiments on heterogeneous hybridization that when 80 or 100 per cent. of the eggs of *purpuratus* formed membranes upon fertilization with the sperm of starfish in hyperalkaline sea-water, often less than one per cent. of the eggs developed into larvæ, while the rest behaved as if only artificial membrane formation had been induced. Last winter the writer and Dr. Gelarie made sure that (as was already indicated by observations of Dr. Elder) only those eggs developed into larvæ in which a sperm nucleus was found, while the eggs which formed membranes without developing did not contain a sperm nucleus. The writer found, also, that when the concentration of NaHO and CaCl_2 used was comparatively high a smaller proportion of the eggs with membranes developed than when the concentration was low. This was easily understood on the assumption that the addition of NaHO as well as of CaCl_2 to the sea-water increased the adhesion of the starfish sperm to the jelly of the sea urchin egg, thus allowing the sperm to induce membrane formation, but preventing or rendering difficult its entrance into the egg.

It occurred to the writer that if this assumption was correct sea urchin eggs which had been deprived of the surrounding jelly by a treatment with hydrochloric acid should all develop when fertilized with starfish sperm and that they should no longer show a mere membrane formation without development. This was found to be true. Sea urchin eggs (*purpuratus*) were deprived of their jelly and several hours or a day later fertilized with starfish sperm in sea-water to which some CaCl_2 and NaHO had been added. Often as many as 50 per cent. of the eggs formed membranes and practically all developed into larvæ; while the eggs of the same female not deprived of jelly when fertilized under the same conditions would all form membranes, but with a very small percentage of eggs developing into larvæ. This indicates that Ca and NaHO may increase the adhesion of the spermatozoa of the starfish to the egg jelly of the sea urchin. It does not prove, however,

that this increase of adhesive power is the factor by which the CaCl_2 and NaHO influence the entrance of the spermatozoon into the egg. It is possible that in addition these two substances also influence the surface condition of the egg by increasing the fluidity of the surface of the egg, thus favoring the spreading of the fertilization cone of the egg around the spermatozoa.

5. The question arises whether or not the addition of CaCl_2 and of bases favors the phenomenon of sperm agglutination¹ caused by the supernatant sea-water of the eggs of the same species, which F. Lillie has discovered. This is not very probable, since the addition of NaHO to sea-water shortens the duration of the agglutination² and therefore acts like an "antiagglutinin." It is true that the addition of CaCl_2 favors the agglutination, but so does the addition of MgCl_2 ; yet the latter substance without the presence of CaCl_2 or the addition of a base does not enable the spermatozoon to enter the egg.

It is, however, possible, if not probable, that some specific substance in the surface of the egg or spermatozoon or of both may also aid in the entrance of the spermatozoon into an egg of its own species. If this be true, in certain cases an excess of alkali or of CaCl_2 may compensate to some degree the lack of specific substances for the entrance of the spermatozoon into the egg, *e. g.*, in the fertilization of the egg of the sea urchin by the sperm of starfish, brittle stars, holothurians and others.

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¹ On the basis of observations on the sperm of *purpuratus* the writer was doubtful whether the specific cluster formation of the sperm caused by the supernatant sea-water of the eggs of the same species was a phenomenon of agglutination or a tropistic reaction. In *Arbacia* the agglutination is much more pronounced than in the case of *purpuratus*. The surface tension phenomena which the writer described may therefore find their explanation on the assumption of an agglutination, at least in the case of *Arbacia*.

² *The Journal of Experimental Zoology*, Vol. 17, page 123, 1914.